REVIEWS.

ART. X. *Memoir of the Life and Medical Opinions of John Armstrong, M. D.: to which is added, an Inquiry into the facts connected with those forms of Fever attributed to Malaria or Marsh Effluvium.* By FRANCIS BOOTT, M. D. Vol. II. London, 1834.

IN a notice which we gave, some time since, (Vol. XIII. p. 425,) of the first volume of this work, we took occasion to examine, somewhat at various length, points which came incidentally under review. The life of JOHN ARMSTRONG, particularly in its earlier period, was out of the ordinary train of scholastic or professional education. It was not strange therefore, that some of his opinions in reference to medical education and medical institutions, should have been at variance with the opinions of those to whom the guardianship of those systems of education and polity have long been entrusted. It seemed to us a suitable occasion to offer some remarks on those peculiar views. They are questions of great practical interest to us, no less than to the medical community of Great Britain. Our own institutions for professional education, and for the cultivation of professional character and attainments, are derived, with only partial alterations, from those of the mother country. Many of these alterations, we believe, indeed, to be great improvements; still the same great principles, in many particulars, run through the systems of both countries. And when they are attacked by a man of so much eminence and worth as Armstrong, it seemed worthy of the inquiry, to consider how far the systems themselves are justly amenable to his censure. The result to which we were brought, was, that so far as Armstrong had cause of complaint, the true cause was to be found rather in the administration of the system, than in that system itself; and that in the irritation of personal feelings he did not always perceive that the cause of the evils of which he complained, if it were not sometimes, in part at least, in himself, was to be found more in an unhappy management of ancient institutions, than in the unfitness of such institutions themselves, to accomplish their legitimate objects, in the protection of the great mass of society from the evils of unrestrained

quackery, and of the profession itself from the inroads of unworthy members.

If in this examination, we have spoken less favourably of Dr. Armstrong in some particulars, or have expressed less deference to his opinions on some points than the more zealous of his admirers would have wished, we have at the same time been not insensible to the many estimable traits of character which his life exhibited. Neither have we, as we trust, undervalued the contributions which he made to medical science, nor the improvements which, in some degree at least, he introduced into medical practice, although we were not prepared to subscribe to the correctness of all of his opinions, and still less to acknowledge the sufficiency of the evidence on which some of them rest. The deference paid to extended generalizations in medicine, is every day diminishing, and the conviction is constantly growing, that far more extensive and more accurate series of observations are necessary, before we can frame theories of disease that shall be entitled to much more of consideration, than as curious or ingenious speculations. Still farther were we from any design, or any willingness, to speak in disparagement of the work which was the occasion of our remarks, or of its amiable and able author. With a mind more logical, and disciplined by a much more systematic course of education and study, than belonged to, or were enjoyed by the master whose opinions Dr. Boott illustrates and enforces, he has brought to his task an extensiveness of research, and an ability of argument, which Armstrong himself could never have given to it. We are aware that this will sound to the ears of the author more like censure than commendation. For there is a fidelity and an earnestness of friendship in all that connects him with Armstrong, which leads him to throw in all his own powers of investigation and reasoning, to sustain the opinions and reputation of his friend. Yet we cannot but feel, and feeling it we see no reason why we should not say it, that he would have produced a better and more useful book, had his connexion with that distinguished man been somewhat less intimate, and his devotedness to his opinions less entire and absorbing.

As it is, it is a work of much interest and value. Although in our view, not a little trammelled by the desire to support the whole system of opinions derived from Armstrong, yet the "Inquiry into the facts connected with those forms of Fever attributed to Malaria or Marsh Effluvium," furnishes a history of febrile diseases, in connexion with the phenomena attending their origin and progress, abounding with information of the most important kind, and highly interesting in its character, even to those who are not convinced by the

evidence it presents, of the correctness of the views which the author entertains, in regard to their true cause.

We have before expressed an opinion of the excellence of this Inquiry, in regard to the fevers of our own country, in the first volume of this work. But the discussion of the preceding topics had so extended our remarks, that we did not speak of this part so fully perhaps, as its value and importance, especially to physicians in this country, deserved. We are not disposed to go into a very detailed examination of it at this time. Yet some notice of it seems necessary, to prepare the way for a satisfactory comparison of the fevers of America with those of Europe, which are the peculiar subject of this second volume.

Before entering upon his general survey of the fevers of this country, Dr. Boott goes into an examination of the question of the identity of yellow fever with the bilious remittent, acknowledged to be indigenous; in which he shows how little foundation there is for the opinion of Chisholm, that a distinct disease, the much talked of *Bulam Fever*, was imported and confounded with it. This examination, able and, to our minds, perfectly conclusive as it is, was scarcely necessary to convince the physicians of this country. We believe few remain, who have any doubts on the subject. For us, as we have before remarked, it is chiefly interesting, as a faithful and judicious summary of the facts and arguments which have long since settled the question. In this point of view this essay possesses great interest; for we hardly know where else can be found so condensed and at the same time so complete an examination of the whole subject.

To British physicians, however, the subject is still of a higher and more practical importance; since to them, at least to a great portion of them, as represented by their medical writers, the doctrine of contagion is a stumbling block, which they seem to have no power to get over. To them, if we might hope that our recommendation would reach them, we would recommend a careful perusal of this chapter; and if their minds are not hopelessly bewildered by the confusion into which the greater part of their writers have fallen, about contagion and infection, they will find a clearness and abundance of evidence which will forever settle the question with them also.

So lately as April, 1835, (such is the date of the publication,) an author, no less distinguished than James Copland, author of the Dictionary of Practical Medicine, makes use of such language as this: "The non-infectionists insist upon four things; the truth of which they *take for granted*, and make the bases of their arguments

in proof of their doctrine. The first, is the passage of fever into plague, and of remittents into yellow fever."—Dict. of Pract. Med. p. 776. The other *three things* are not particularly connected with our present topic, and we pass them without notice. But those who are familiar with the fact and the proof of it, that remittent fever not merely passes into yellow fever, but that the latter is no more than an aggravated form of the former, may be surprised to learn that all this is only "*taken for granted!*" They may perhaps account for the assertion by the supposition, that not only all American papers on the subject, but also the works of such British writers as Bancroft and Boott, are ranked by the author of it among "inferior publications," and as such not only, as his prospectus tells us "excluded from his Bibliography," but also from his reading. Neither of these works are noticed in the Bibliography to the article "Epidemics," nor in that of "General Fever," though both of them, and especially that of Dr. Boott, are peculiarly appropriate to the former, and by no means inappropriate to the latter. As the article "Remittent Fever" is not completed, it does not yet appear whether they may not receive some notice under that title.

The survey of the fevers of the United States begins with the fevers of the southern states, then passes in review those of the middle states, and concludes with those of New England. It every where exhibits a great degree of industry and research in seeking out the best sources of correct information, and of ability in selecting and condensing them into a uniform and consistent and instructing history. This history, is chiefly a history of the epidemic fevers of the North American States; for the obvious reason, that it is only of the epidemics that any materials exist, sufficient for the construction of any thing like a connected history. Few physicians take sufficient notice of the diseases of their ordinary every-day practice, to record and preserve, still less to publish, an account of them. We have indeed a few exceptions to this remark, and the publications which constitute them are among the most valuable parts of our medical literature. But, in general, our published accounts of diseases refer only to those extraordinary periods when diseases have prevailed to such an extent as to assume more or less of an epidemic character.

It need not be supposed, however, that this circumstance materially affects the correctness of the history, in reference to the more ordinary diseases of the country. So far as the nature of its diseases is concerned, a history of its epidemics is probably a true history of all its diseases. That concentration of the cause of a disease which converts it from a sporadic into an epidemic, may also operate in the

individoal cases to render them more severe; bot it is not likely to change their character, so as to give rise to essentially a different disease.

The first epidemic noticed by Dr. B. in this survey, prevailed at Waynesborough, in the interior of Georgia, in 1802. It was a remittent fever, in some cases, entirely intermitting. As Dr. White, from whose paper in the New York Medical Repository the accoont is taken, says of it, "It may be called a simple remittent; a bilious remittent; a malignant bilious, or a yellow fever. They are only different grades of the same disease, arising from the difference of constitotion, and avdiversity in the natore of the predisposing and exciting causes. I do not hesitate to declare I have seen since my settlement here, many well-marked cases of yelluw fever, though the yellowness was not always an attendant symptom. At the distance of a hondred miles from the nearest sea-port, it is farcical to look for foreign sources of infection."—Vol. I. p. 307. Is this to *take for granted* the "passage of remittents into yelluw fever?"

This accoont is fullowed by a notice of a similar epidemic in Augusta, Georgia, in the soumer and aotomn of 1804; then by a short notice of diseases in the British army near New Orleans, in 1814 and 1815, and in the American army in the same neighbourhood in 1809. For the porpose of exhibiting other examples of disease of acknowledged endemic origin, the author next presents us, thoogh somewhat inaccoratcly, in reference to his geographical divisiun, with accoonts of epidemics at several places on the Ohio River, in 1796, 1797 and 1800, and several later years. The same purpose is pursoed by giving an account of a fever on the Roanoke River in 1792, and at Greenville, on the Tar River, in 1800, both in the interior of North Carolina.

"In these observations of fever," he remarks, after quoting in some detail the histories of the several authors, "as it occurs in the interior of North Carolina, we find intermittents and remittents prevailing in the usual seasons, seldom fatal to the natives of the country, but more formidable to strangers,—typhus attacking the former in dry seasons, spring, or summer, sometimes epidemic, and referred distinctly by Dr. Pitt, to a concentrated marsh effluvium; and in the winter and spring, a continued fever, certainly of a specific kind, according to Dr. Williamson's observations, attacking those who have been afflicted with intermittents, and prevailing within certain limits, chiefly, perhaps only, in those places where people are subject to intermittents, in low sunken grounds and along the sides of rivers." Vol. I. p. 348.

He next torns to the sea-coast, and gives us from Dr. De Rosset, an account of yellow fever at Wilmington, North Carolina, and from Dr. Norcom, at Edenton in the same state; and much more at

length from Dr. Ramsay and Dr. Johnson, of the same disease, in different years, in Charleston, South Carolina. This is followed by a history of the same epidemic at Norfolk, Virginia; and at Alexandria and Georgetown, in the District of Columbia. At the same time that this disease prevailed in these two latter places, 1814, a similar epidemic raged at Winchester, Virginia, and Fredericktown, in Maryland, and many other places in the interior of that state, as well as of Virginia and Pennsylvania. This epidemic is compared with that of the sea-board, and the conclusion is irresistible, not in the mind of Dr. Boott merely, but with the authors of the several accounts, the actual observers of the disease as it prevailed under their care, that it was substantially the same disease with the yellow fever, as it prevailed in the cities on the coast.

The chapter on the fevers of the "Middle States of North America," is made up chiefly with accounts of the yellow fever at Philadelphia, in 1793, 1798, and 1802. These epidemics are too well known to all our readers, who have given any attention to the subject, especially that of 1793, by Dr. Rush's most interesting history of it, to render it necessary for us to notice them in detail. With these, Dr. B. compares the yellow fever of Wilmington, Delaware, in 1798; and an intermittent and bilious remittent fever which prevailed extensively in 1804, in the interior of Pennsylvania, and several other of the middle and southern states. This last epidemic does not appear to have risen to the grade of yellow fever, but to have exhibited many of the same general characteristics, only in a milder form.

One circumstance is to be especially noticed here, in reference to our author's views of the cause and character of fever. In the more southern states, it had already been observed, the fevers in the summer and autumnal months, uniformly assume the intermitting or remitting form; and it is only during the winter months that those of a continued type are ever found. In the middle states, continued fever is found in some sporadic cases, even during the summer and autumn, in those years in which no extensive epidemic prevails. As we advance farther north, the prevalence of continued fever increases, and that of the periodical types declines, until in New England the latter appear only at rare intervals, in years of an extraordinary high temperature, by which the climate is for the time assimilated to that of more southern states. The development of this fact, going to show the identity of the cause of typhus fever, with that of intermittent and remittent fevers, is the leading object of the whole history. There is a remarkable confirmation of this view of the subject, in an occurrence, given on the authority of Dr. Rush, which took place in

Philadelphia in 1801. A number of emigrant families arrived at Philadelphia in August, from Ireland and Wales, and brought with them the ship, or typhus, fever. They were carefully attended at the Lazaretto and the City Hospital, and the disease did not spread. "Contrary to its usual character," says Dr. Rush, "it partook of the remissions of the bilious fever, probably from the influence of the season upon it." We must give Dr. B.'s remarks on this occurrence entire.

"The fact of the ship fever, or typhus of Great Britain, changing in the month of August, at Philadelphia, to a remittent fever, is too curious and interesting to be passed over in silence. Two explanations of this change of type may be offered."

"1. That the fever originating in Ireland, Wales, or on ship-board, from contagion, according to the commonly received opinion of the source of typhus, remained as such so long as no other remote cause of disease existed; but that, on the arrival of the patients in Philadelphia, they became exposed to the operation of malaria, which gave rise to a new type of fever, viz. the remittent."

"2. That the typhus, originally a disease of malaria, changed, on landing, from a continued to a remittent type, from the influence of climate, and of those circumstances which contributed to improve the condition of the sick, and consequently to restore them gradually to health."

"The idea of two distinct causes, viz. a specific contagion and marsh effluvium, uniting to produce a compound fever, has been entertained by Pringle and other authors; and it arose from the circumstance of fevers occasionally exhibiting the characters of typhus and marsh fevers,—beginning as the former, and ending as the latter, or the reverse; and from the belief that neither of the causes alone could give rise to the conjoined symptoms."

"If it can be shown, that in situations obnoxious to malaria, and where no source of typhus contagion exists, or can be reasonably suspected to exist, a fever appears, exhibiting the intermittent, the remittent, and the continued or typhoid character, and that the milder form passes into the more severe, or the severe into the milder, it is almost a necessary inference, that these types are effects of malaria, and consequently, no great surprise would be felt at the fact of typhus changing into a milder form of fever, from the sudden influence of such favourable circumstances as patients labouring under it on ship-board experience, when they are landed in a genial season, placed in clean and airy apartments, their wants and comforts zealously attended to, and their minds cheered by the termination of the voyage, &c. It is well-known that continued fevers have a certain duration; and that when once formed they must, under the same circumstances of situation, go through a regular rise, progress, and decline; and we could not, therefore, expect an immediate termination to the ship fever on its arrival in Philadelphia. But though it did not cease, it put on a milder character, or at least it remitted, so that the chain of febrile action was broken; and Rush admits that no proofs of contagion were manifested."

"I have already shown that typhus has appeared in different places in America, where malaria abounds, and where the periodical types of fever are the usual consequences; and I would especially refer to the epidemic at Wilming-

ton, and to the consideration of those slow nervous or chronic states of fever, which Rush frequently mentions as arising out of the yellow fever, or coëxisting with it. These I consider as very analagous to, if not identical with, the typhus of Great Britain; and if these indigenous fevers of America sometimes arise out of, or pass into, a periodical type, I see nothing anomalous in a similar fever imported from Ireland or Wales exhibiting a change from a continued to a remittent fever." Vol. I. pp. 490—492.

We might easily object to some of the views here presented, but we prefer to pass our objections by, and go on with the survey of American fevers. The fevers of the city and state of New York, furnish many favourable opportunities for a satisfactory investigation of the question as to their origin, of which our author has availed himself, with his usual ability. But for the reasons already given, in regard to those of Philadelphia, we need not follow him in his detailed examination of them. Most of our readers are too familiar with the subject, to require it of us; and those who would examine it anew, we refer to the work itself. We cannot, however, refrain from presenting the testimony of Dr. Coventry, which is referred to in this chapter, though quoted in another part of the work, as a specimen of the manner in which the "passage of remittent fever into yellow fever," *is* "*taken for granted.*"

"After my removal," says Dr. C. "to the flat country, called the Lake Country, I met with fevers, in which I often recognised the black vomit and intense yellowness described in the yellow fever. Acquainted with the diseases prevailing in the cities only through the medium of their newspapers, I had conceived the disorder raging there as one of a very different type, and resembling what I conceived to be the plague of the Levant, more than the epidemic that was so generally spread over the western district of the state of New York, and with which I had become so well acquainted, having in my own family, and subsequently myself, undergone the disease. But a visit to New York, in the autumn of 1805, undeceived me; for in that part of the city near the wharves, and bordering on the East River, I recognised on the first glance, the cast of countenance with which I had been but too familiar since the 20th of July preceding. It happened also that the most intimate friend I had in the city, resided in a low situation called Pearl street, and had remained there during the season. He had the yellow fever himself; also three of his family, two of whom were victims. It was conceded by the gentleman who attended this family, that these were genuine cases of yellow fever. I noted, from the reports of my friend, confirmed by the testimony of his physician, the symptoms; and found every one correspond with those of many patients who had fallen under my care, none of whom had been near the sea-coast, and most had never seen any thing larger than a canoe or small boat in their lives. After the most close attention, I was as fully convinced of the identity of this disease with the epidemic that had ranged along the margin of the western lakes, as I was of my own existence." Vol. I. p. 279.

The eastern states furnish but a few examples of the prevalence of yellow fever as an epidemic. In 1798, it prevailed to a considerable extent in New London, Connecticut; and in the same year, and also at two or three other periods in Boston, Massachusetts; and to a smaller extent in a few other places. Other forms of fever, however, have prevailed to a greater or less degree throughout New England. The history of these, so far as he could find materials for it, Dr. Boott has collected and compared. In this way, besides the accounts of the epidemics of yellow fever, already referred to, he passes in review, the diseases of Rhode Island, as recorded by Dr. WHEATON, those of Vermont by Dr. GALLUP, those of the northern army during the war of 1812, and many others. The general result of the whole investigation into the origin of the fevers of America is thus stated:—

"In taking, therefore, a general review of the diseases of North America, which owe their origin to malaria, we observe yellow fever almost annually epidemic at Vera Cruz, lat. 19° 11′; and that it gradually disappears in frequency as we approach the latitude of Boston, 42° 20′; that in the parallel of Philadelphia it often ends in typhus; that north of this line, typhus becomes the more frequent form of fever, till we lose all trace of yellow fever in Vermont, as a distinct disease, the symptoms of both, however, being sometimes blended in the same case; that both of them are attended, from the south to the north, with diarrhœa, dysentery, *cholera infantum* in the summer, and in the winter and spring with a pleurisy or peripneumony, which in the south is more decidedly marked, like the autumnal fever, with bilious symptoms; sometimes, as Rush observed in 1803, with a tendency to a tertian type; and in the north, having, like its fevers, a more typhoid character, sometimes approaching to the most malignant forms of congestive fever. It is highly interesting to observe, that when typhus occurs in those parallels of latitude obnoxious to yellow fever, it is towards the decline of that disease, as the cold weather sets in. In the north it occurs at the same season as yellow fever does at the south, though later generally, at least as an epidemic. Dr. Gallup says its greatest prevalence in Vermont is from July to September; but that it extends into the early part of winter, and sometimes till spring. This analogy with the typhus of Great Britain is important. It has been too confidently asserted, that the typhus of this country is confined to winter."

"Of the causes which aggravate the fevers of cities, I think no impartial person can doubt, from the general contrast afforded, in this inquiry, between the epidemics of the sea-coast and those of the interior.

"The confined limits of the disease; its prevalence in low alluvial districts; the striking origin of the epidemic at New London; the coïncidence of cause and effect in Boston and New York; the occurrence of fever on ship-board, under circumstances like those described by Dr. Burnett—all point to local causes of contaminated air; and this will explain the prevalence of fever in the close, crowded, and filthy part of the towns and cities of this country.

"That yellow fever cannot be considered a contagious disease, is proved beyond all question. It is not communicable by the sick, in domestic life or in

hospitals. The matter of black vomit has been swallowed, and applied to the body in a variety of ways, without any effect. We have seen that the typhus of New York and Boston, was as incommunicable there as yellow fever; and Dr. Gallup's experience in Vermont was uniformly against any thing like a contagious character." Vol. I. pp. 614—616.

Our readers will now be prepared to follow our author over a similar survey of the prominent febrile diseases of Europe. Here the range of country is much less extensive than that which we have gone over, and the influences upon disease, to be expected from differences of climate, are much less. But the length of time which the history embraces is much more extended, and the variations in the condition of the inhabitants in respect to the comforts of life, and to exposure to disease, are vastly greater; and hence we are not to be surprised if the forms and appearances of fever should prove to be more diversified, even although they should still be found to proceed from a common origin.

The most formidable of the early diseases of Europe is the plague; and with this Dr. B. begins his second volume. His purpose is thus stated.

"I shall examine some of the facts connected with plague, and see how far it can reasonably be considered a marsh fever. By this term I mean intermittent, remittent, continued, or mixed form of fever, arising from malaria, or, contaminations of air, aggravated in their effects by atmospherical influences, and considered under all its modifications as essentially one disease, variable in character, especially in different seasons, climates, and situations; desultory in the locality of its prevalence, generally obedient to particular seasons, its worst forms, arising more or less gradually out of the milder, or passing into them; generally attended with something unusual in the character of the year of its greatest virulence; and falling most severely on the lower classes of society, whose situation, habits and circumstances, favour its early development and diffusion among them." Vol. II. p. 9.

He first speaks of the plague of London of 1665. It may be asked what means we have of ascertaining the character and origin of that disease, other than the declarations of those who witnessed and have described it? It is apparent that Sydenham and his contemporaries who have written the history of the plague of 1665, regarded it as an imported disease, and believed that it owed its destructive ravages in a great degree to its contagious character. The question is asked, somewhat contemptuously, by Copland, "did the writers who lived subsequently know more of the matter than Sydenham and others, who saw the commencement of that plague, as well as its decline?"*

* Dictionary of Practical Medicine, p. 775.

The answer is easy. We do not rely upon any ancient author, nor indeed upon any author, for the peculiar opinions he may have expressed, but for the facts and arguments upon which those opinions are grounded, or rather for information on which to found our own. A peculiar bias of mind, or the prevailing notions of the day, may have influenced an author's speculations, and given a character to his opinions, which the facts he relates do not support. A subsequent writer therefore, having the advantage of the accumulated observations of later times in regard to kindred diseases, as well as the opportunity of comparing those of contemporaneous authors, may draw his inferences from the facts they have detailed, with a hope of coming to a right result in respect to its origin, better even than could well have been indulged by those who saw so much of its ravages. Especially may such a result be anticipated from a careful comparison of the history of its rise and progress with that of other diseases of acknowledged character and origin, which immediately preceded, and accompanied, and followed it. This is precisely what Dr. Boott has done with the London plague of 1665.

Intermittent and remittent fevers had prevailed in London for many years previous to 1665; and these were of so severe a character that the number of deaths ascribed to ague and fever, in the bills of mortality, from 1659 to 1664, amounted to from 2,107 to 3,490 in a year; and in the fatal 1665, the number attributed to this cause is 5,257. It was about the middle of this year, 1665, that the true plague began, or at least that it was recognised as such. It was at its height in September, continued in some degree through the winter, and disappeared the following spring. The number of deaths reported in the bills of mortality was in 1665, 68,596, and in 1666, 1,998.

In the spring of 1665, a few months before the plague made its appearance, *as plague*, an epidemic fever prevailed extensively, which Sydenham regards as of a different character from the fevers of several previous years, belonging to a different "constitution." It was a violent disease, and often fatal; and partook so much of the character of true plague as to be often called by Sydenham, the *pestilential fever*. "Whether it deserves to be entitled *a plague*," says Sydenham, "I dare not positively affirm; but this I know by experience, that all who were then seized with the true plague, attended with all its peculiar concomitants, and for some time afterwards, in my neighbourhood, had the same train of symptoms, both in the beginning and through the course of the disease." In his directions for the treatment, he classes this pestilential fever with plague, and re-

commends the same treatment for both. This is sometimes spoken of as "the malignant fever," and sometimes as the "spotted fever." It appears to have been recognised by this last name in the bills of mortality. In the tables of these bills, furnished by Dr. B., we find 1,998 deaths recorded by spotted fever in 1665. And from these tables it would seem not to have been regarded as a new disease in that year. A considerable number of deaths by it appear in the bills for a long series of years both before and after that time.

It will at once be seen, that the true character of this pestilential, or spotted fever, standing as it does in so close a relation to the acknowledged plague, is of great importance in ascertaining that of the plague itself. The descriptions of Sydenham and others, are not very full in regard to it. Yet it does not appear that he looked upon it as very different from the fevers which had preceded it, and he gives no intimation of any suspicion of a foreign origin; while the plague, according to Hodges, was imported in bales of merchandize from Holland. Dr. Boott regards this fever as substantially the same disease as the plague itself, differing from it only in the degree of violence with which its symptoms were manifested: in the same manner as yellow fever is a more aggravated form of the common bilious remittent, and like that, arising from a greater concentration of the same cause. This view of the matter is supported in a very able, not to say conclusive manner, by a history of the progress of the two forms of disease.

One of the ablest and most zealous of the advocates of the doctrine of contagion, Dr. Copland, has recently advanced a similar opinion in regard to the identity of this fever with plague; and it is not a little curious to see two men referring so confidently to the same circumstance for the support of conclusions so entirely opposite. "Upon a careful examination of Sydenham, Baynard, Hodges, De Foe, &c." says Dr. Copland, "it is manifest that the malignant spotted fever, said to have been prevalent at the commencement and decline of the plague, was actually this distemper, reported as this fever for the purposes of concealing its existence; and that where this fever actually existed, it was one of those forms that plague very commonly assumes, especially during low ranges of temperature, as at that season."*

We might here retort upon Dr. Copland his own question, whether "subsequent writers know more of the plague than Sydenham and others;" especially as this point refers to the character of the disease

* Dictionary of Practical Medicine, p. 776.

which was the subject of immediate observation, and not like the other question, to a matter of general inquiry. But we are too well pleased with the opinion itself to quarrel with the author of it, about the inconsistency of his opinions. We may ask, however, if this spotted fever were the true plague, what becomes of the importation of plague from Holland, in the summer of 1665? And if plague were called spotted fever merely for the purposes of concealment, why was the latter designation continued, after the former had become recognised and acknowledged? Still more confidently may we ask, what becomes of the contagion of plague, since this form of it continued to prevail in London every year from 1647 to 1728, and to occasion a considerable number of deaths each year? Indeed this question may be asked with great force, in regard to acknowledged plague. That disease had existed in London, and been distinctly recognised as such in the bills of mortality, nearly every year for almost a hundred years. Yet it had prevailed extensively as an epidemic only in a few years, generally at distant intervals; and it never appeared as such after 1666, although it prevailed extensively in some parts of England, for several years later. If so contagious, why should it have ceased so suddenly? Not, Hodges tells us, for the want of subjects. Nor was it for want of free communication between the well and the sick. For we are told, that although the alarm was great at first, it speedily subsided, and there was afterwards great fearlessness in respect to intercourse between them.

The truth appears to be, that the two forms of disease were indeed essentially the same in character; and that a distinction was made between them, not as Copland supposes, for the corrupt purpose of deception, but because the minds of the several authors who described it were so impressed with the notion of the necessity of contagion to make up the character of true plague, that they were unwilling to give the name to a disease so obviously indigenous, and to which no appearance of contagion attached itself. How often have we seen the same sort of distinction attempted in our own day, in regard to yellow fever, with scarcely any pretence of any difference of character as exhibited by the phenomena of the disease, and resting exclusively upon a supposed difference of origin.

The conclusion to which our author arrives in this part of his subject, may be seen from the following extracts:—

"Since 1670, the decrease of fever," in London, as shown in detail by a table, which we have not room to copy, "has been progressive from 3,423 to 932 annually, a mortality very inconsiderable, considering the amount and the quality of the population. Bowel complaints," as appears from the same table,

"have steadily decreased since 1670, and the disproportion in modern times, 20 to 2,966, is certainly a most remarkable fact. Heberden justly insists upon this as an evidence of an endemic cause of disease during the seventeenth century, when plague was almost annually present in London from 1603 to 1679. I had intended to have offered his cogent reasoning on this subject, but I shall content myself with referring to his valuable paper.* It is singular to observe the state of his mind and that of Hancock, after their investigation into the probable origin and progress of fever, and to contrast their evident doubts and cautious inferences with the dogmatical assertions of those who take no trouble to inquire into the subject. It is very evident that both these excellent men more than doubted of the introduction of any foreign contagion as the cause of plague; which even if admitted to be possible, on the shallow evidence that is brought in its support, cannot account for the previous existence and the simultaneous increase of other diseases, to say nothing of the absence of any new importation since 1679, notwithstanding the notorious frequency of plague in the East, and the constant and increased communication with it, especially of late years, since cotton has become an article of import from Egypt. If quarantines have saved us, how comes it that plague has never appeared in those establishments? And if contagion could act as the cause of an epidemic, why was not the tragedy of 1593, 1603, 1625, 1636, and 1665, acted over and over again, especially in years when there died in London from 600 to 4,240 of plague?" Vol. II. pp. 78, 79.

"That plague in those years in which the mortality was trifling, was but the peculiar aggravation of the ordinary fever of London, is, I think, apparent from the limited amount of the disease. In speaking of the pernicious fevers of Italy, as described by Torti, I have remarked, that the peculiar cases of aggravation which are met with in any one epidemic, are sometimes found to predominate in particular epidemics; and plague is a proof of the fact, for, like the occasional examples of yellow fever, of dysentery, and choleric fever, scattered in sporadic cases through epidemics generally of a common remittent character, we find equally sporadic cases of plague in the epidemics of the fevers in this country; and occasionally these diseases at other times predominate over all other forms, and the other fevers are then the exceptions to the prevalent yellow fever, dysentery, cholera, or plague. Upon what this depends it is impossible to surmise; though I see no difficulty in conceiving, that if the ordinary causes of fever can produce scattered examples of yellow fever in plague in some years, they may produce them generally in others, without the necessity of resorting to the forced supposition of an imported contagion. This, I think, must be abandoned with respect to yellow fever and typhus; and I cannot see how, consistently, it can be maintained with respect to plague, nor can I imagine that the doctrine of a contagion, *sui generis*, is applicable to either of the diseases." Vol. II. pp. 80, 81.

Dr. Boott next proceeds to speak of plague, as it has been described by different authors, "tracing it from south to north, with a

* "Observations on the Increase and Decrease of different Diseases, by W. Heberden, Jr., M. D. London 1801."

view to observe whether temperature has any modifying effect upon it, analogous to what we observe in marsh fever." Assalini, who accompanied the French army into Syria in 1799, describes what he calls "an epidemic fever," which attacked the troops engaged in that expedition. He seems to have given it that term, for the same reason that we have before noticed in regard to others. Having imbibed the notion, that the plague is necessarily a contagious disease, he was unwilling to give the term to a disease which obviously did not possess that property; although he says, that from the havoc it caused, it was called the plague. From his description of the disease too, it is impossible to distinguish it from plague as described by others. The severe cases were accompanied by buboes and carbuncles, although these were sometimes wanting. But this is no more than what has happened in other epidemics of plague. As Dr. B. justly remarks, it may be seen from the facts that he states of plague, that, "it is a disease which owes its fatality to the symptoms common in other modifications of fever; and that abstract those conditions dependent on fever, and consider it merely as an eruptive disease, and its formidable character disappears."

Assalini's description of the symptoms of this fever is not very particular, and it does not appear whether it remitted, or was strictly continual. But the plague of Aleppo in Syria, of 1742 to 1744, as described by Dr. Russell, was preceded and accompanied, and followed by a fever, analogous it would seem, to the spotted fever of London, which often was a true intermittent. It is curious to see in the mind of Dr. Russell, the same bewildered distinction between this fever and plague, which we have before repeatedly noticed, founded upon the assumption of the necessity of contagion to the character of plague. Yet the different forms of the disease were so blended together, as to render it impossible to distinguish them by the symptoms. For example, in 1743, "autumnal intermittents became frequent about the beginning of August, got to their height in September, then gradually decreased, and disappeared at the close of the year." These fevers at the beginning, often assumed for a few days a continued form, with violent and irregular symptoms, *not unlike those of the plague;* but after bleeding and purging, they reassumed their genuine forms of tertians, double tertians, and quotidians, and were cured by bark. The next year, intermittents existed from the middle of March to the beginning of May. "In June, July, August, and a part of September, *a malignant fever prevailed, attended with much the same symptoms as the plague*, buboes and carbuncles excepted." "The cure was much the same as in the

plagoe, only that the sick bore a seeond bleeding better." From Jone to December, autumnal intermittents were very frequent. They did not, like those of the preceding year, begin with the appearanee of a continued fever; but unless they were earlier eured by bark, they were apt after the seventh day to intermit no more, but to ron oot under a continual form to the fuurteenth or twenty-first day, unless fatal at an earlier period.

Another strong analogy between the epidemies, of which the plagoe forms a part, aod the fevers of Ameriea of aeknowledged indigenous origin, on which Dr. B. insists with much force of argoment, is foond in the similar character of the diseases whieh prevail doring the eolder portiuns of the epidemic periods. He had before shown, although in the eoneiscuess of our analysis we may not have notieed it with suffieient prominence, that during the aotomn and winter aod spring of those years in which yellow fever was epidemic, diarrhœas and dysentery and peripneomony were unusually prevalent. The same was true at the time of the plague in London, particularly in the memnrable winter of 1665–6. And we find the same diseases mentioned among the spring epidemies of Syria.

To eontrast the plague of Egypt and Syria with the same disease in a more northern latitude, Dr. B. quotes that of Nimigoen in 1636, deseribed by Diemnoeek, in which the fever was generally of a continued type. We need not, however, follow him in this examination, as we have done in those whieh preceded it. Oor readers will, by this time, have seen with what ability and indostry, and we think we may add fairness, he has investigated the different epidemics under examination. In one particular, however, they will do him great injustice, if withuut any qualifieation they were to found their opinion of his work opon the analysis of it which we have presented. Our extracts and remarks have had chief reference to the topic, which may be considered indeed as the leading subjeet of the work—the origin and cause of fever. It would have led us too far, and exteoded this artiele moch beyond its proper limits, to have noticed at the same time the rich information with whieh these volumes abound, in regard to the partieolar eharacter of the diseases which they describe, and the phenomena exhibited by them. In this point of view, a high valoe and interest must attach to the work, independently of its influence upun the long agitated question of the euntagion of diseases.

Dr. B.'s notice of the celebrated plague of Marseilles of 1720, which has been so much relied on in proof of the contagious eharacter of the disease, is brief, but is marked by his usual research and

ability. The result to which he arrives, may be seen from the following extract.

After an examination of the circumstances which were said to accompany the introduction of the disease, as related by different authors, he says—

"The inference that the plague was not owing to contagion, is supported, 1st, by the fact, that cases probably occurred in 1719, but certainly in April and May, 1720, before the ship arrived from Syria, which was accused of bringing the contagion; 2d, by no disease having been communicated, to all appearance at least, at Leghorn, where she touched on her passage to Marseilles, and where three of her crew died; and 3d, by the plague itself, in many cases, not differing from the usual endemic fevers of the south of France, excepting in the frequent, but *not invariable* occurrence of the bubo or carbuncle." Vol. II. p. 246.

Our author next turns to the ordinary forms of fever, tracing them in the works of some of the more distinguished medical authorities, from Italy and the coast of the Mediterranean, through Paris to Great Britain.

"It will be found," he says, "that the periodical type is almost universal in the south, and that the continued is the representation of the same disease in the north; that if the last ever distinctly occurs in Italy, or the first in Britain, they are *exceptions to a general rule.*"

He first gives a sketch of the fevers of Italy, from the works of Torti and Lancisi; and then an account of the petechial fevers of the same country, from Fracastorius, Sarcone and Acerbi.

"The work of Torti," he remarks, "is especially valuable for the minute details of the protean forms of marsh fever, though his observations were made in too southern a latitude, viz. at Modena, lat. 44° 34′, for the unmixed form of continued fever, as it occurs generally in Britain, to have been presented to his view. He considered that all the varieties, from the mild to the most malignant intermittents, and from these, with distinct intermissions, to the continued form of fever, were modifications of one disease; and he fully verifies the accuracy of Dr. Armstrong's observation, that the types of fever pass and repass into each other; that inflammation, as an internal condition, forms, to all appearance, no part of the intermittent type, and that its supervention is one of the circumstances which gives rise to the continued form. He also shows, what I have endeavoured to exemplify in the sketch of the fevers of America, that temperature has a marked influence on the type of fevers, their tendency to the continued form increasing as the cold of autumn deepens into winter." Vol. II. p. 250, 251.

Of the petechial fevers of Italy, he says—

"It is evident, that the modern Italians consider it identical with the typhus of Britain, and perhaps uniformly ascribe its origin and diffusion to contagion. I refer those who are curious as to the history of the disease, to the second

chapter of Acerbi's work.* I shall content myself with offering proof, that in Italy it is the aggravation of marsh fever, and distinctly arising out of the periodical type; a mode of development which harmonizes with what I have advanced of the influence of high temperature, and with the agency of malaria." Vol. II. p. 329.

To give in detail the several facts and arguments by which the author illustrates and supports these several positions, would demand much more copious extracts from his work than our limits will permit; we must content ourselves therefore with having presented his own conclusions, and refer our readers to the work for the proofs by which he sustains them. We have already exhibited too much evidence of his elaborate research and sound reasoning for them to doubt that he has powerful, if not satisfactory and conclusive grounds for his positions. With the same remark, we must pass over the sketch of the fevers of Malta and the Ionian Islands, taken chiefly from Hennen's Medical Topography of the Mediterranean, and that of the Fevers of Minorca from Cleghorn. An examination of them, that should be of any avail, would require more of details than we have left ourselves room for.

Indeed, we must extend the remark still further to our author's examination of the typhus of Paris and of that of Great Britain. It does not admit of condensation, so as to be presented in the form of an abstract, without entirely destroying its value. We could wish to present to our readers his analysis of Louis' observations and researches in regard to the pathology of typhus; but we cannot enter upon it at the close of so long an article. We are not without the hope of returning to this part of the subject at some future time, especially if we shall find the means of comparing the observations of that great man with the results of similar researches in regard to the character of that disease in other places.

We cannot conclude without repeating the remark, that our readers will greatly mistake, if they should infer from our notice of this work, that it is limited to the establishment of a particular hypothesis. It is truly a general history of febrile epidemics, abounding in information of the most valuable kind, and narrated in a harmonious and pleasing style. We are well aware, that a history designed to illustrate opinions previously adopted, is liable to be drawn aside into partial views of the subjects of which it treats. But we have in the history before us, constant marks of candour and fairness, sufficient to set our minds at rest, in respect to any intention of mislead-

* "Dottrina del Morbo Petechiale. F. E. Acerbi. Milano, 1822. pp. 132."

ing us; and if we suppose our author to be sometimes insensibly influenced as to the weight of his authorities, by his preconceived opinions, the abundance of the details which he presents us, affords a ready corrective to the supposed partiality. Independently of the peculiar views which these volumes are designed to support, they have brought together a great mass of information of inestimable value, which it would be quite impossible to find elsewhere, except indeed by the same laborious process as that to which the author has himself resorted. E. H.

Art. XI. *Rapport et Discussions à l'Académie Royale de Médecine sur la Taille et la Lithotritie, suivis de Lettres sur le même subject.* Par MM. Delmas, Souberbielle, Rouchoux, Civiale, Velpeau. 8mo. Paris, 1835. pp. 194.

In the last number of this journal we offered some remarks on the work of M. Blandin, entitled *Parallele entre la Taille et la Lithotritie*, and we ventured to intimate, that with all its avowed candour, the author was evidently somewhat of a partisan. Nothing could tend to show the powerful, we might almost say violent party spirit at present prevailing in Paris, between the defenders of the two modes of operating to which we have alluded, than the debate in the academy, on the report of M. Velpeau.

Four long sessions were occupied in discussing, we know not precisely what—for the question at issue is nowhere defined with clearness. The debate winds up with "conclusions in which nothing is concluded"—for the report was finally adopted amid "incredible tumult," and numerous protests against the manner of voting; M. Lisfranc demanding that the question should be put again at the succeeding session. There was much sharpness of retort and some little personality displayed in the debate, and among other after results, we may notice a warm discussion between MM. Velpeau and Civiale, of which we discover, as yet, but the *beginning of the end.* The damps of three thousand miles of ocean have a wonderful effect in cooling the warmth of an argument, and we shall endeavour to extract some few facts and conclusions from the several sources above enumerated, in order to illustrate the opinions at present entertained in France on the value of lithotripsy.

The memoir of M. Leroi contains the history of five cases in which the operation of lithotripsy was performed by him on children